OUR BOOK SHELF

Meibauer's Physische Beschaffenheit des Sonnensystems. (Berlin: Carl Habel.)

THIS is a second and freshly-arranged edition of a comprehensive little treatise on the nature of the solar system. It requires no great acquaintance with the present state of science to vindicate the accuracy of the author's preliminary remark, as to the difficulty that students experience from the wide dispersion of modern observations among heterogeneous memoirs and journals in various languages, and the necessity of a large library and abundance of leisure; and it is impossible not to appreciate his attempt to combine these scattered materials in a condensed and accessible form. Nor can it be doubted that a considerable amount of labour has been devoted to the work, which has been made attractive by perspicuity of treatment and facility of style, as well as by occasional ingenuity in hypothesis. Yet the execution cannot be said to correspond with the excellency of the design; and the deficiency, more apparent perhaps to our own minds than to those of Continental readers, is such as necessarily results from one-sided and imperfect views. The eternity of matter, an idea to many minds especially and utterly abhorrent, should not, to say the least of it, have been assumed; and other less objectionable hypotheses and statements are adopted, which may not be as incontrovertible as unwary readers will be led to suppose. No doubt the author, in employing as part of his motto the words of Darwin, "False facts are highly injurious to the cause of science," was quite unconscious that the result of an inquiry into some of his own facts (or rather assertions) would not be quite satisfactory. But we do not know what to make of such statements as these—that Priestley called his vital air (oxygen) by the name of Phlogiston—that Huggins found in the nuclei of comets the lines of nitrogen, hydrogen, and carbon similar to those given by the Geissler tubes—that there are two bright lines in the spectrum of Sirius, one of which is displaced by the star's movement—that the red, green, and yellow tints of the aurora never lose their relative positions; that the force of gravity at the upper limit of the atmosphere may be considered not materially different from that on the earth's surface, while the centrifugal (tangential) force perceptibly increases. Nothing but an unkind, or bitter, or self-ignorant spirit would refuse to leave a fairly broad margin for inevitable human imperfection; but it must be a very large paper copy indeed that would find room for statements such as these. Nor is it easy to understand why Lockyer's just claim should have been ignored to an equal share with Janssen in the grand discovery of prominences round the uneclipsed sun; or why discredit should have been thrown upon the connection of the solar-spot maximum with Sabine's magnetic period, or the planetary one established by the Kew observers. Other omissions might be pointed out, and the work would have been greatly improved by a discussion of the effects of temperature and pressure in modifying elementary spectra—a branch of inquiry to which allusion has barely been made, but which is of essential importance in spectrum analysis, and the fuller development of which alone, perhaps, promises a more satisfactory solution of many cosmical phenomena. But while it appeared a matter of duty to mention these deficiencies, we must add, in all fairness, and with greater pleasure, that some of his theories are very interesting and well handled; such as that in which he would account for the eruption of the protuberances by the unstable condition of gaseous matter on the confines of fluidity, discovered by Andrews and Thomson; or that of the unlimited extension through space of the planetary atmospheres in extreme tenuity; and there is much ingenuity, at any rate, in the idea of accounting for the variations of atmospheric pressure and electricity between the tropics by the resistance, however infinitesimal, which our globe sustains in its rapid passage through a space to which neither Newton nor Laplace ascribed absolute vacuity. The curious inconsistency with which, as a denier of equivocal generation, he calls in the germs of terrestrial vegetation from external space, where they have been educed under certain conditions of temperature, pressure, and time, is but a specimen of the difficulties to which every hypothesis is subject, that ignores the existence of an omnipotent will; but there are some who will look with amusement, and some few with a warmer feeling, at his vigorous onslaught on the idea of a luminiferous æther; concluding with the keen remark, that to prove the existence of such an æther, recourse is had in turn to the very phenomena which it was invented to explain.

T. W. W.

Knapsack Manual for Sportsmen on the Field. By Edwin Ward. (Bradbury and Evans, 1872.)

ONE who has come so much in contact with sportsmen as Mr. Ward must have done should surely know that men do not go out with knapsacks when intent on killing big game. The title "Knapsack Manual" is most unfortunate. Moreover, if the book is intended for sportsmen *on* the field, why should a considerable portion of it be given up to the mode of setting up a tiger, which a sportsman is very unlikely to do for himself, and certainly would not attempt in the field? Mr. Ward, though he seems to have considerable regard for artistic treatment and compatibility in the setting up of skins, would yet appear to put lichens with his stuffed birds in the conventional style. What a relief to the eye it would be to see a case of stuffed birds without a particle of dead wood or lichens in it! The directions given for skinning and preserving specimens are not full enough; there are better works on the subject in existence. The lists of game to be found in various parts of the world, at the commencement, form the most useful part of the book. The account of a Museum of Natural History of the Earth from man to a granite stone contained in a case 10 ft. long by 7 ft. high, displays a lamentable amount of ignorance. Some of the remarks about the various creatures are very amusing, as, "Gasteropoda proceed by the belly." "Armadillos are very remarkably swift in flight." Altogether this book appears to be of the nature of an advertisement, and we think a not very successful one.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

The Method of Least Squares

IN the number of NATURE for June 6, Prof. Asaph Hall, of Washington Observatory, called attention to what he regards as a singular oversight in the history of this subject, viz., that in 1770-1773 Lagrange published an elaborate memoir at Turin under the title "Mémoire sur l'utilité de la Méthode de prendre le Milieu entre les résultats de plusieurs Observations, &c." Prof. Hall remarks that the only notice of this memoir he has seen is contained in the *Berliner Jahrbuch* for 1853, and that in the abstract of a memoir of mine on the subject in the notices of the Royal Astronomical Society for April 1872, the name of Lagrange does not appear.

As regards myself, I need only state that Lagrange's memoir, as well as Simpson's, is referred to in my paper; although, as no examination is made of it there, the name is omitted in the Abstract, where reference is only made to the authors of investigations in which an attempt is made to prove either the law of facility or the method of least squares, and which were therefore referred to with more or less detail in the paper itself.

Further, I should not regard it as an omission if in the history of Least Squares no mention was made of Lagrange; in fact,

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when I was examining all the investigations I could find on the subject, after looking through Lagrange's memoir (and reading carefully Todhunter's *résumé* of it), I came to the conclusion that it contained nothing that could, properly speaking, be regarded as an anticipation of the later investigations of Gauss, Laplace, &c., and I contented myself therefore with merely a passing reference.

Lagrange's paper, as its title implies, gives a mathematical justification of the choice of the mean of a series of discordant observations, and a determination of the chance that the resulting error lies between certain limits, with developments, &c.; but the method of Least Squares may be described as an extension of the principle of the arithmetic mean to the combination of linear equations, involving more than one unknown; the problem being to obtain the best values of the unknowns from a series of discordant *linear simultaneous equations.*

The method of Least Squares was first proposed in print by Legendre in his "Orbites des Comètes" (Paris, 1805), as a convenient way of treating observations without reference to the Theory of Chance. Legendre's words are "la méthode qui me paroît la plus simple et la plus générale, consiste à rendre minimum la somme des quarrés des erreurs . . . et que j'appelle Méthode des moindres quarrés. The method, regarded from a practical point of view, is a very natural one; we shall clearly get a good result by determining the quantities to be found so as to make the sum of the $2n$th powers of the errors a minimum, and in order that the resulting equations may be linear (and therefore manageable), we must take n equal to unity.

Though first published by Legendre, the rule was applied by Gauss, as he himself states, as early as 1795, and the method is explained and the usual law of facility for the first time found in the "Theoria Motus Corporum Cœlestium, Hamburgh, 1809 (not 1808, as in Prof. Hall's letter). The principle on which Gauss proceeds may fairly, I think, be stated as follows:—If there are given a number of discordant observations V_1, V_2, &c., of a quantity x, so that we have the equations $x - V_1 = o$, $x - V_2, = o$, &c., then it is known that a very good result is obtained by giving to x the arithmetic mean of its observed values, and writing $x = \frac{1}{n} (V + \ldots + V_n)$; and it is required to find an equally good rule for determining x, y, z, &c., from a number of discordant equations of the form $a_1x + b_1y + c_1z + \ldots = V_1$, $a_2x + b_2y + c_2z + \ldots = V_2$, &c. Assume therefore that $x = \frac{1}{n} (V_1 + \ldots V_n)$ is the most probable value of x derived from the first system of equations, and find the law of facility of error that this may be the case; then, having this law, the most probable values of x, y, z, &c., can be found for the second system.

The law of facility Gauss finds to be represented by $\frac{h}{\sqrt{\pi}} e^{-h^2x^2}dx$, viz., this is the chance of an error of magnitude intermediate to x and $x + dx$; and thence it follows that the most probable values of x, y, z, &c., are found by making $(a_1x + b_1y + c_1z + \ldots - V_1)^2 + (a_2x + b_2y + c_2z + \ldots - V_2)^2 +$, &c., a minimum. Gauss then proceeds to determine h in the manner still generally adopted.

Subsequent writers, Laplace, Poisson, &c., have in consequence investigated how far the arithmetic mean is the most probable result, &c., and in one sense Lagrange (and *a fortiori* Simpson) may be said to have very slightly anticipated a portion of the analysis required in these researches, although, as far as the method of Least Squares is concerned, there is no anticipation. A slight examination will show how greatly superior Laplace's analysis is to Lagrange's on the same subject.

With reference to the independent discovery of the method of Least Squares by Dr. Adrain of New Brunswick, U.S. (see Prof. Abbe's note in the *American Journal of Science*, June 1871), I may remark that if for distinction we call the introduction of the merely practical use of the rule its "invention," and its philosophical deduction by the Theory of Probabilities its "discovery" (so that Legendre invented the method and Gauss discovered it), then Dr. Adrain can only be credited with the independent invention of the rule, viz., he only did what Legendre had done two years previously. This is worth noticing, as from the occurence of the function e^{-x^2} in Dr. Adrain's paper, it might be supposed that it contained some anticipation of Gauss' investigation; but such is not the case, and Dr. Adrain's reasons for the adoption of the law are of so trivial a nature that it is incredible that any mathematician should have been led to the discovery of the method by means of them. I imagine that he had noticed the practical convenience of the rule, and subsequently endeavoured to justify it analytically; it may be noted that it is possible that Dr. Adrain may have seen or heard of Legendre's memoir published two years before; his silence on the matter, however, renders it unlikely that this was so. On the whole, by far the greater part of the merit of the introduction of the method is due to Gauss; while the credit of the first suggestion of the practical rule must be assigned to Legendre, Dr. Adrain having, in all probability independently, also suggested the same rule subsequently. It is necessary to be thus particular, as Gauss' publication having taken place in 1809 and Adrain's in 1808, it might be thought that the latter had anticipated the former to some extent, which is in no wise the case.

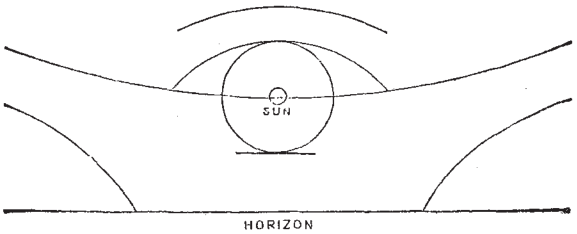
In writing the history of the Theory of Errors or the Theory of the Treatment of Observations, there are several memoirs anterior to Legendre's that would have to be included, and notably Thomas Simpson's "Miscellaneous Tracts," 1757 (which is the work Prof. Hall doubtless refers to), Daniel Bernoulli's "Dijudicatio maxime probabilis plurium observationum discrepantium," &c. Acta. Petrop. 1777, Trembley's paper in the "Berlin Memoirs," 1801, "Observations sur la méthode de prendre le milieu entre les observations," &c. For the above references I was indebted to Todhunter's "History of the Mathematical Theory of Probability from the time of Pascal to that of Laplace" (London, 1865), which contains a notice of every work or memoir on the subject to the commencement of the present century (there is a *résumé* of Lagrange's memoir occupying 13 pages), so that no one need have any fear of passing over any writings published previously to 1800. Having had occasion to make much use of the work, I may be permitted to say that its value, both as regards accuracy and completeness, cannot be over-estimated.

J. W. L. GLAISHER

Trinity College, Cambridge, June 8

Solar Halos

A BEAUTIFUL combination of solar halos was visible here during the morning of March 2. At 10·45 the sun having an altitude of about 40° was surrounded by a complete rainbow-tinted circle of some 18° or 20° radius, red inside and blue outside. An arc of a larger circle coloured in the same way touched the complete circle at its highest point, rendering the point of contact dazzlingly bright. A short arc touched the lowest point of the circle in the same manner. A white halo passed through the sun's position parallel to the horizon, and two fainter white arcs intersected it obliquely in the point opposite to the sun, forming a conspicuous sun-dog. There were also two rainbow-arcs having their convexities toward the sun. These were blue inside and red outside, and their centres appeared to be about 90° from the sun, and some 15° below the horizon. Later an arc concentric with that touching the complete circle appeared above it, having the colours reversed, namely, blue inside and red outside. These appearances lasted about an hour and a half before beginning to fade away.



W. W. J.

Gambier, Ohio, March 5

The Volcanoes of Central France

THE Auvergne volcanoes threaten to be as periodic a subject of controversy as the authorship of the letters of Junius. It is only seven years since the last eruption of letters. At that time I contributed a paper to the *Geological Magazine* (vol. ii. p. 241), in which I collected, printed, and translated all that I could find on the subject, and came to the conclusion that it was very probable there had been some local outbreak of volcanic action. Thus I agree with Mr. Garbett, but it appears to me that he has not

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